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BOOK REVIEWS

Untersuchungen über die Natur der griechischen Betonung. Von
HUGO EHRLICH. Berlin: Weidmannsche Buchhandlung, 1912.
Pp. xi+275. M. 8.

The unparalleled conservatism and transparency of the Greek vowel system has generally been regarded as a result of the musical accent—or, more accurately, of the level stress—of that language. In late years, however, several scholars have assumed rather extensive influence of the Greek accent upon the phonology. Shall we, then, conclude that Greek had after all a fairly strong stress accent from very early times, or shall we take a skeptical attitude toward the utilization of the facts of Greek accent for the explanation of sound changes?

This is the question that Ehrlich has set himself. Although he clearly prefers the second alternative, he goes about the examination of the evidence quite judiciously. He does not even cite the great intrinsic probability that a language with a remarkably conservative vocalism did not have enough stress to affect the phonology at all.

As an example of his method we may consider the treatment of apocope in the first chapter.

A tabulation of all the occurrences in Homer of the prepositions *ἀν(ά)*, *κατ(ά)*, and *παρ(ά)* not in composition leads to the formulation of this law: a succession of three short vowels, separated by single consonants, is avoided by the suppression of the middle one if that stands at the end of a word. There are only 53 exceptions out of a total of 1,203. The same prepositions in composition with verbs show 88 exceptions to the rule out of a total of 649 occurrences. The greater irregularity here is plausibly explained as due to leveling within the paradigm: *καθήμεν* led to *καθήσω*, etc. Most of the apocopated forms in noun compounds conform to Ehrlich's law. He admits exceptions in *κάμμορος* from **κατ-μμορος*, *ἐπασσύτεροι* from **ἐπαν-σσύτεροι*, *ἐπητής* and *ἐπητής* from **ἐπ-γητ-*.

The restriction of the monosyllabic forms to certain rhythmical surroundings is usually supposed to indicate that apocope was a matter of tradition with the epic poets and was used only on account of metrical necessity. Hence it was confined to the thesis where short syllables are impossible. Ehrlich finds this explanation inadequate because it does not account for (1) the lack of scansions such as *παρὰ Διός* - - - (like *εἰν ἀγορῇ, ὑπὲρ ἄλλα*), and (2) the rarity of phrases like *ἄμ πύργους* - - | -. He is surely over-

stating his case here: there is no difficulty in supposing that the poets avoided $\pi\tilde{\alpha}\rho$ where they could, but nevertheless preferred it to $*\pi\tilde{\alpha}\rho\acute{\alpha}$, and the phrase $\alpha\tilde{\nu}\alpha$ $\pi\acute{\upsilon}\rho\gamma\omicron\nu\varsigma$ can be got into the verse by merely putting the preposition in the arsis. Ehrlich's other objection to the appeal to metrical necessity has more weight. Apocopated $\pi\alpha\rho$ and $\alpha\nu$ were perfectly good Ionic forms ($\pi\alpha\rho$, *SGDI* 5434, 9, etc.; $\alpha\nu$ -, *Hdt.*, *Herodas*, etc.), and the poets would not have avoided them.

The Homeric exceptions to Ehrlich's rhythmic law are easily accounted for as the earliest extensions of the shortened forms. The full forms have scarcely spread beyond their original sphere. They can, of course, stand before single consonant and short syllable only in case of metrical lengthening (e.g., $\kappa\alpha\tau\alpha\kappa\epsilon\acute{\iota}\alpha\tau\alpha\iota$), and this occurs only three or four times. Of the other dialects, only Aeolic-Thessalian is as near the primitive stage as Homer in respect to apocope. All the others show extensive modifications in its use.

It was analogy which confined the operation of Ehrlich's law almost entirely to a few prepositions; $*\pi\alpha\tau\acute{\epsilon}\rho$ for $\pi\alpha\tau\acute{\epsilon}\rho\alpha$ beside $\pi\alpha\tau\acute{\epsilon}\rho\alpha\varsigma$ could not survive in view of $\tilde{\alpha}\nu\delta\rho\alpha$ beside $\tilde{\alpha}\nu\delta\rho\alpha\varsigma$, etc. It is, however, still possible to find a few stereotyped case forms that have lost a final short vowel. Thus $\tilde{\alpha}\gamma\kappa\acute{\alpha}\varsigma$, "in the arms," is plausibly explained as a locative plural of $\tilde{\alpha}\gamma\kappa\acute{\omega}\nu$. $\tilde{\omicron}\phi\rho\alpha$ and $\tau\acute{\omicron}\phi\rho\alpha$ may well contain $*\tilde{\omicron}\phi\iota$ (from $*\tilde{\omicron}\phi\iota$) and $*\tau\acute{\omicron}\phi\iota$, instrumental adverbs. Since the final syllable of the first member of a compound is treated as an absolute final, $\tau\alpha\nu\alpha\acute{\iota}\pi\omicron\delta\alpha$ must come from $*\tau\alpha\nu\tilde{\alpha}\rho\acute{\omicron}\pi\omicron\delta\alpha$. $\phi\epsilon\rho\acute{\epsilon}\varsigma\beta\iota\omicron\varsigma$ from $*\phi\epsilon\rho\epsilon\sigma\acute{\iota}\beta\iota\omicron\varsigma$ is a formation of the type of $\acute{\epsilon}\lambda\kappa\epsilon\sigma\acute{\iota}\pi\epsilon\pi\lambda\omicron\varsigma$. Ehrlich discusses several similar formations.

While one can hardly say that our author has proved his case in regard to apocope, still his theory is decidedly the most attractive one that has yet been presented. It is certainly preferable to J. Schmidt's notion that apocope was a result of proclisis.

A similar verdict must be passed on several sections of the book. But Ehrlich everywhere shows that the accentual theories which he combats are both unsatisfactory and unnecessary. We shall probably hear no more of a stress accent¹ in Greek prior to the fourth century B.C.

Our author, however, credits the evidence which has been adduced to show that the stress accent began during that century (pp. 149 ff.). By far the strongest of this evidence consists of four clip-forms from the dialect of the lowest classes in Athens. The comic poet Amphis ridicules a fish dealer for saying, $\tau\acute{\tau}\acute{\alpha}\rho\omega\nu$ $\beta\omicron\lambda\acute{\omega}\nu$ and $\kappa\tau\acute{\omega}$ $\beta\omicron\lambda\acute{\omega}\nu$, and the verb, $\sigma\kappa\omicron\rho\alpha\kappa\acute{\iota}\zeta\epsilon\sigma\theta\alpha\iota$, *Dem.*, presupposes the phrase $\sigma\kappa\omicron\rho\alpha\kappa\alpha\varsigma$. We must probably admit that in the fourth century certain individuals at Athens used a stress accent.

Ehrlich attaches more weight to Kretschmer's well-known argument that the confusion of ϵ with η and of \omicron with ω (Ehrlich adds $\epsilon\iota=\tilde{\iota}$ with $\tilde{\iota}$) in inscriptions and papyri indicates a breaking-down of the distinction

¹ Here and elsewhere, by "stress accent" is to be understood, of course, a fairly strong stress accent, such as might be expected to affect the phonology.

between long and short vowels, and therefore a relatively strong stress accent. The reviewer has shown (*TAPA* XLII, 45 ff.) that the confusion between ϵ and η and between \omicron and ω was really due to the loss of the distinction in quality which had originally led to the use of two characters for the e -sounds and for the o -sounds. It is probable that the occasional use of ϵ for η is similar to this.

The third piece of evidence for a stress accent in the fourth century is a law which was first pointed out by Kretschmer, *Wochenschr. f. kl. Phil.* 1899, 5, to the effect that an unaccented vowel in the neighborhood of a nasal or a liquid is dropped if the next syllable contains the same vowel. Kretschmer assumes the operation of the law from early times, but Ehrlich finds the first certain instance of it in $\sigma\acute{\kappa}\acute{o}\rho\delta\alpha$ for $\sigma\acute{\kappa}\acute{o}\rho\omicron\delta\alpha$ in Crates of Thebes, who flourished at Athens about 328 B.C. The next example is $\beta\epsilon\rho\nu\acute{\epsilon}\iota\kappa\eta\varsigma$ for $\beta\epsilon\rho\epsilon\nu\acute{\epsilon}\iota\kappa\eta\varsigma$ in *Pap. Petr.*² 1. 2. 7 (237 B.C.). There is no doubt about the validity of Kretschmer's law, but we may still hesitate to accept it as evidence for a stress accent. As Ehrlich acutely remarks elsewhere (p. 2): "Nicht einmal ein Lautwandel, der auf unbetonte Silben beschränkt ist, muss in jedem Falle eine Wirkung schwacher Expiration sein." It is clear that we have before us a law of dissimilation rather than of syncope, and who can say, in the present state of our knowledge, whether or not a purely musical accent can determine which of two vowels is to be lost by dissimilation? There is therefore no objection to admitting such of Kretschmer's earlier instances as are plausible in themselves. For example, $\pi\lambda\acute{\epsilon}\theta\rho\omicron\nu$ is more likely to be a younger form of $\pi\acute{\epsilon}\lambda\epsilon\theta\rho\omicron\nu$ than a different ablaut grade. (In this case and in some others one may doubt whether the accent played even a secondary rôle.)

In the fourth place, the author discusses the "weakening" of unaccented α to ϵ in the neighborhood of ρ . The phenomenon appears, from the third century on, in such words as $\acute{\epsilon}\rho\rho\eta\phi\acute{o}\rho\omicron\iota$ (for $\acute{\alpha}\rho\rho\eta$ -), $\kappa\alpha\theta\epsilon\rho\acute{\iota}\zeta\epsilon\iota\nu$, $\mu\upsilon\sigma\epsilon\rho\acute{o}\varsigma$, etc. But among the instances cited are $\acute{\alpha}\mu\phi\acute{\iota}\epsilon\rho\alpha\omicron\varsigma$ (five times in Attic inscriptions) and $\acute{\rho}\acute{\epsilon}\phi\alpha\nu\omicron\varsigma$, where the altered vowel is accented! It is difficult to see why we should suppose that the accent had any connection at all with the change.

There is no convincing evidence for a stress accent in Greek before the Christian era, except in the speech of some very low social stratum at Athens in the fourth century.

Ehrlich treats in detail a number of points which concern his main thesis only indirectly, and these excursions embrace some of the most valuable contributions in the book. We may examine one which is devoted to an important point in Latin grammar.

Our author's elaborate argument in favor of Ebel's hypothesis that the Thessalian genitives in $-\omicron\iota$ are locatives in origin may or may not win general approval. Much more attractive is a theory which he advances in this connection in order to rescue the old identification of the genitive and locative

singular of the Latin *o*-stems (pp. 71 ff.): "Während in der ältesten (lateinischen) Sprache unbetontes *oi* und *ai* gleichwie betontes *ei* sich in \bar{i} wandelten, wurde unbetontes *ei* zu reinem \bar{i} ." In that case early genitives like *Latini* and *urbani* (SC. *de Bacchanalibus*) and contracted genitives like *Pomponi* in the dramatists are no longer evidence for an original \bar{i} . In Celtic also it is possible to trace the genitive ending \bar{i} to earlier $-ei$, and the Messapian $-ihi$ in the genitive of *io*-stems may represent earlier $-iei$.

Ehrlich is able to point out several incidental advantages of his new theory: we are no longer compelled to assume that Oscan $-eis$ in the genitive of the *o*-stems represents a transfer from the *i*-stems; it is the Italic genitive-locative in $-ei$ plus $-s$ which is the final sound of the genitive singular in all other stems (cf. early Lat. *isti-s*, etc.).

Kent has demonstrated (*AJP* XXXII, 272 ff.¹) that Lucilius' rules for the use of *ei* and *i* are in general accurate, but he felt compelled to admit an error in the line (369 Marx): "hoc illi factum est uni," tenue hoc facies *i*; for *illi* and *uni* could not be separated from Oscan *altrei*. On Ehrlich's hypothesis Lucilius is right here too.

Another point deserved rather more space than Ehrlich has allotted to it. Although *quoiei*, dative singular of *qui*, does not occur in inscriptions until after the confusion of *ei* and \bar{i} had become general, it is usually assumed that the original form of the case was really $*quo\bar{i}iei$, with $-ei$ which stood in some relation to the ending of the dative singular of *iste*, *unus*, etc. It has, however, been difficult to derive the various historical forms from this (cf. Husband *TAPA* XLI, 22). The theory of Sommer, *Handbuch*, pp. 465 f. (cf. Exon *Hermathena* XII, 218), that $\bar{i}\bar{i}$ of $*quo\bar{i}iei$ dropped before following \bar{i} , which then united with the preceding *o* to form a diphthong (cf. gen. sing. *Pompēi* from $*Pompēi\bar{i}\bar{i}$), could not be accepted while it was supposed that original unaccented $-ei$ was pronounced $-\bar{e}$ in Plautus' day. Now, however, the difficulty is removed; $*quo\bar{i}iei$ regularly became $*quo\bar{i}\bar{i}$ > $*quo-\bar{i}$ > (Plautine) *quoi*. (So far we have been following Ehrlich.) The spondaic *quoi-i* which occurs several times in Plautus (Neue-Wagener II, 454; Sommer, *Handbuch*, p. 465) and epigraphic *quoiei* are re-formations on the basis of the genitive. The reviewer does not recognize the "Plautinische Messung $qu\bar{o}i\bar{n}$ " which Ehrlich cites (p. 77). If there really is any such form, it should rather be spelled $qu\bar{o}\bar{s}-\bar{i}$ and explained in the same way as $\bar{e}\bar{i}$ (below).

The history of the dative of *is* runs parallel; $*ei\bar{i}ei$ regularly developed into monosyllabic *ei*. Spondaic $ei\bar{i}\bar{i}$ is due to the influence of the genitive. The iambic form probably arose from the analogy of *isti* and the other pronominal datives; *istum:isto:isti=eum:eo:e\bar{i}. That the early dramatists used both the spondaic and the iambic forms of the dative of *is* there is no doubt, but for monosyllabic *ei* the evidence is not so satisfactory. None of the passages cited by Neue-Wagener (II, 378 f.) really demands a*

¹ One misses a reference to this article on p. 73.

monosyllable; that pronunciation has metrical guaranty only in Cat. 82. 3 (hexameter):

eripere *ei* noli multo quod carius illi
est oculis

But if there was a monosyllabic dative of *is* in Catullus' time there may have been one a hundred and fifty years earlier. The reviewer has shown (*Contraction in the Case Forms of the Latin io- and iā-stems*, p. 18) that the nature of early prosody prevents our distinguishing a long monosyllable from an iambic word in the iambic and trochaic verses of the early dramatists unless it occurs (1) as the final syllable of an iambic cadence, or (2) as the second syllable of a resolved thesis or arsis, being shortened by the iambic law, or (3) where the meter requires the complete loss of the word by elision. Consequently our failure to prove the presence of such a form in the dramatic poets does not create a presumption that it is non-existent. We need have little hesitation about reading *ei* as a diphthong where, for example, it balances the monosyllabic genitive, as in Ter. *Ph.* 188: "Efus me miseret, *ei* nunc timeo, is nunc me retinet. . . ." The point with which we are at present concerned is that this monosyllabic *ei* is the regularly developed dative parallel with *quoi* and *hoic*.

There are, of course, some difficulties in the way of assuming that unaccented *ei* became *-ī* in preliterary times, but they are none of them fatal. The locatives *Brundisii* (Enn.) and *Sunii* (Ter.) beside contracted genitives such as *ingeni* were long ago adequately explained by Lachmann: "illud *i*, quod est ante notam casus, Graecum videbatur, ut more Latino praeteriri non posset." Cato's *Lavinī* is in all probability correctly recorded.

Tibei, etc. (*sibei*, SC. *de Bacch.*), can no longer be directly connected with Osc. *sīfei* and Paelignian *tfei*. The Latin forms, like OB. *tebē*, *sebē*, contain Indo-European *-bhoi*, while the Umbrian and Paelignian words go with OPr. *tebbei*, *sebbei*, and show the *e*-grade, *-bhei*. The reduced grade of the suffix appears in Greek *-φει*.

Since the dative singular of consonant stems is found with diphthongal spelling several times before the sound *i* begins to be written *ei* (e.g., *virtutei*, CIL I, 30), it is necessary to give up Solmsen's (KZ. XLIV, 161 ff.) connection of it with Oscan *medikei*, etc., on the basis of an Indo-European dative ending *-ei*. This is the more regrettable in view of *recei* in the old Forum inscription. If this form contains a dative suffix *-ai*, we must assume that the inscription is later than the weakening of unaccented *ai*.

Ehrlich is probably wrong in assuming (p. 72) on the basis of Plautine *dī*, *dīs*, *ī*, and *īs* that in dissyllabic words even unaccented *oi* and *ai* had become *ī* in Plautus' day. It is far simpler to suppose that the sound group *-eē* contracted to *-ē*, which later became *-ī*. The forms *dīi*, etc., were merely orthographical representations of *dī*, etc. (See my *Contraction in the Case Forms of the Latin io- and iā-stems and of deus, is, and idem*, pp. 15 ff.)

Ehrlich's treatment of his subject is independent and suggestive throughout. Even where his conclusions fail to be accepted, his novel grouping of the evidence will lead to numerous advances in our knowledge.

Misprints are very few. The only one of any consequence that has been observed is in l. 3 of p. 28, where one should read $\alpha - | \alpha$ for $\alpha | \alpha$.

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I Fasti Consolari Romani. By GIOVANNI COSTA. Milan: Libreria Editrice Milanese, 1910. Vol. I, Parts 1 and 2. Pp. x+547; 150.

L'Originale dei Fasti Consolari. By GIOVANNI COSTA. Rome: E. Loescher & Co., 1910. Pp. 77.

These books are a monument of zeal and patience if not of wisdom. In the preface of the first the author states that he proposes to initiate a "radical and fundamental" reform in the study of Roman history by substituting a consistent inductive method for what he regards as the more or less haphazard method of criticism hitherto in vogue. With this laudable purpose in mind he devotes this volume to an exhaustive study of the lists of magistrates of the republic, intending in the second volume to restore the original, which he calls the *spina dorsale* of Roman history, and to draw such conclusions as may be based upon it. It is perhaps unfair to pass final criticism before the constructive part of the work is in our hands, but it is at least proper to express grave doubt as to the surpassing value of this *spina dorsale*. It is not of great importance, for our real appreciation and understanding of the development of the history of Rome, that we should know whether a Fabius or a Claudius was consul in any particular year. So our eagerness to know what revolutionary use Costa will make of such facts when determined is all the more keen.

His theory is in brief this: In literature and on the monuments we have two distinct traditions of the list of magistrates of the first three centuries of the republic, both derived from the records of the pontiffs, the *Annales pontificum*. One, which he calls the chronographic, was that of the revised form of the *Annals*, published by Q. Mucius Scaevola, and known as the *Annales maximi*. This was the basis of the chronicles of Nepos and Atticus and of Varro's work, and is preserved for us with varying degrees of accuracy in Diodorus, Cicero, Velleius, the *Fasti Consulares* and *Triumphales* on the Regia, Tacitus, Cassius Dio, Pliny, and the *Chronograph* of 354 A.D. The other tradition was based on the *Libri lintei* made up from the *Annales pontificum* in the latter part of the fourth century B.C. This was a simple list, and preserved for the earlier period a greater degree of authenticity than the more elaborate *Annales* themselves, which suffered at the hands of editors